

and of the ground, at a depth of 1 inch, $80^{\circ}9$; 9 inches, $80^{\circ}7$; 20 inches, $82^{\circ}6$; 60 inches, $83^{\circ}8$; and 132 inches, $83^{\circ}2$. It is desirable that the errors of these underground thermometers were ascertained.

Down to the close of 1864 the hourly observations made at Bombay were published *in extenso*, and these twenty-four years' hourly observations furnish data for the prosecution of many inquiries, the value of which it would be difficult to over-estimate. From 1865 to 1872 the individual observations ceased to be published, but the hourly means for the different elements continued to be published. From these the hourly means of pressure, temperature, humidity, cloud, thunderstorms, &c., can be obtained for a period of more than thirty years. From the beginning of 1873, however, no hourly observations, or even hourly means, appear in the reports, want of funds presumably being the cause of the omission. Irrespective altogether of the length of time over which the observations have been made and the immense value this single consideration gives to the Bombay observations, the position of this observatory with respect to the monsoons and other vital elements of the meteorology of India render the maintenance of a first-class meteorological observatory in this part of the empire indispensable. It is in truth simply necessary in the interests of Indian meteorology and its satisfactory development that the Bombay Observatory be kept in a state of high efficiency, and that the individual observations made there be published and distributed among men of science at least as liberally as they were previous to 1865.

OUR BOOK SHELF

Supplement to "Euclid and His Modern Rivals," containing a Notice of Henrici's Geometry, together with Selections from the Reviews. (London: Macmillan and Co., 1885.)

WE noticed the original work at such length in these columns (NATURE, vol. xx. p. 240), that it is not worth while on the present occasion to do more than draw attention to the issue of this "Supplement."

Prof. Henrici's "Congruent Figures" was published nearly contemporaneously with Mr. Dodgson's book, and so he was unable to discuss the methods employed by the Professor, who, in the words of the present preface, "fills the rôle of that popular functionary, dear to Parisian diners, *le quatorzième*."

The discussion forms scene vi. of Act ii., and is headed "Treatment of Parallels by Revolving Lines," and an extract, as usual, leads the way from Henrici's Art of Dining (so our humourist puts it), viz. "in order that an aggregate of elements may be called a spread, it is necessary that they follow continuously."

It will thus readily appear to the readers of the "Euclid and his Modern Rivals," or of our account referred to above—which by the way is honoured by a partial reproduction amongst the review-selections—that Mr. Dodgson is still himself, and that his hand has lost none of its former cunning. We should have liked him to have given his opinions on other parts of the Professor's book, but it has not seemed good to the author so to act, and he has confined himself mainly, if not entirely, to the Lobatschewky treatment of parallels. With two such combatants now fairly in the arena, we shall be content to act as a mere onlooker whilst the strife wages fiercely between them, eagerly noting the parry and the thrust, and ready, if need be, to use the sponge as this or that combatant is struck.

It might be a mighty pretty encounter—Modern Treatment versus the Euclidian.

Mr. Dodgson inserts remarks here and there in the text of the reprinted criticisms: he does not notice that a complaint he makes against us was in great part apologised for on p. 404 (vol. xx., see above).

Leitsaden bei zoologisch-zootomischen Präparirübungen. Von A. Mojsisovics Edlen von Mojsvár. 2nd ed. (Leipzig, 1885.)

WE are glad to welcome a second edition of this work, which is a very useful manual for museum curators and for demonstrators in the rapidly increasing number of zootomical laboratories. Although it appears to be designed for use in high schools we cannot think that it is likely to displace the manuals already in use in this country: it wants the didactic character of Huxley and Martin's "Elementary Biology," the simplicity and directness of Prof. Milnes Marshall's admirable little book on the "Frog" (which is, we are glad to learn, to be soon followed by others), or the detailed directions of Prof. T. J. Parker's "Zootomy." We may note by the way that these works appear to be unknown to our author, whose knowledge, indeed, of English works on anatomy, or, as Messrs. Wilder and Gage call it, anatomical technology, is very incomplete.

So far as German authorities on "Museologie" are concerned, the second edition appears to have been brought up to date; some additions, not always, however, improvements, have been made in the illustrations; some of the English authors whose works are neglected would have provided the author with a better figure of *Astropecten* than the shocking "representation" which is copied from Bronn. When the third edition is called for we hope we shall find the grave, but perhaps the only important, defect which we have noted corrected and accounted for.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

On Watering the Coal-Dust in Mines

REFERRING to an explosion that happened at Lievin Colliery in the Pas de Calais on January 14 last, my friend M. Ed. Sauvage, Ingénieur des Mines, writes as follows:—"Some experiments have been lately made at Lievin Colliery (Pas de Calais), where a disastrous accident happened a few months ago. I do not think any report of these experiments has been published; but they found the coal-dust inflammable, and the watering of the ways in the mine has been resorted to as a precaution against future accidents."

Twenty-nine persons were killed by this accident, that is to say, all who were in the mine with the exception of one. The survivor, a miner named Cornet, and one of his comrades, had prepared a blasting-shot for dynamite, and called upon the shot-firer to ignite it. The latter examined the place, pronounced it to be free from fire-damp, and lighted the fuse.

At the inquest Cornet stated that he saw the shot go off, and had just time, by a quick movement, to throw himself under a heap of straw lying near when the explosion took place. He remembered nothing more, and attributed his escape to the partial protection afforded by the straw.

After investigating the case and hearing Cornet's evidence, the Government engineer and those of the Company who owned the mine came to the conclusion that the explosion was caused by the ignition of the coal-dust that had been lying upon the timbers which formed the supports of the gallery. In corroboration of this opinion they pointed out the fact that the current of air which swept through the gallery in which the explosion originated was too swift to admit of firedamp lodging there.

For some years past a system of more or less careful watering has been practised in some of the largest and driest of the steam coal collieries in South Wales as a precaution against explosions—and the recent occurrences at Usworth, Lievin, Camphausen, and Pendlebury seem to show that similar measures are greatly wanted elsewhere.

Should watering the dust (locally in the neighbourhood of blasting-shots, or generally in the workings) ultimately prove to be the panacea for great colliery explosions, then it is obvious that the responsibility for the holocausts that are now occurring lies almost as heavily upon those who, having the power, fail to hasten its adoption, as upon those who continue to offer it a selfish or factious opposition.

W. GALLOWAY

The Colours of Arctic and Alpine Animals

I MUCH regret that I have been too busy to reply to my friend, Mr. A. R. Wallace (NATURE, April 16, p. 552), till the present moment, but this delay, unavoidable on my part, is the less to be regretted, since it has given an opportunity for the interesting facts recently adduced by Sig. Lorenzo Camerano (NATURE, May 28, p. 77) to be taken into consideration. As Mr. Wallace, with that keen penetration so familiar to all who know him and his writings, goes to the root of the matter under discussion and raises a distinct issue, I will now beg permission to offer a few words in reply to both these gentlemen.

First, with respect to the physical side of the question, Mr. Wallace is perfectly correct in supposing that colour *per se* has no influence upon the radiating or absorbing powers of bodies as far as regards obscure radiation. But I would point out that in the present case we are not concerned with colour alone; we have not merely to consider whether black or white is the best radiator, but we have for comparison two surfaces, hair or feathers, as the case may be, having, as far as we know at present, the same structure, and differing only in colour. The question before us is whether this colour-difference in the same substance is associated with any difference in radiating or absorbing power, and the final answer can only be given by carefully conducted experiments. I may add that I have long been waiting for an opportunity of conducting the necessary investigation, and with aid that has been kindly offered from several quarters I hope before long to be in a position to arrive at some satisfactory conclusion. The form of experiment suggested by Mr. Wallace, although decidedly worth the trial, does not appear to me to be very safe, inasmuch as the natural structure and arrangement of the fur would be lost in the process of weaving into cloth. Mr. Wallace's strictures as to the use of artificial dyes are, however, quite sound, and in these I fully concur. I may further state that when this question was raised some years ago, I searched literature (although by no means exhaustively) to see whether any experiments had been recorded, and although many hundreds of observations upon the radiative and absorptive powers of different bodies have been made by various physicists from the time of Franklin downwards, I have not been able to find any experiment bearing directly upon the question under consideration.

The point to be decided is, not only whether dark hair or feathers are better radiators than white hair or feathers, but whether the radiative power of these white coverings is less for that particular kind of radiation which is most greedily absorbed by the substance (snow) among which the animals have to pass their winter existence. Till this problem is solved physically we have, as it seems to me, only the purely biological considerations to fall back upon.

Before passing on to the more strictly zoological side of the subject I should like to disclaim the notion to which Sig. Camerano's letter may give rise, that the radiative (as distinguished from the protective) theory of Arctic colouring is original as far as concerns myself. With respect to the white covering of the warm-blooded animals, this theory was, as far as I knew at the time, original when first broached in 1880; but Lord Walsingham afterwards showed that the same conclusion had been arrived at in 1846 by Craven, with whose name it should be more fairly associated. The application of this theory (in a reversed sense) to explain the melanism of Arctic insects is entirely due to Lord Walsingham, and as my friend Mr. Wallace is disposed to give the weight of his authority to this extension of the theory, there is no occasion to discuss this point further on the present occasion.

It now remains to point out some of the considerations which

have led me to the belief that the protective theory of white colouring is not wholly sufficient. Thus, among birds there seems to be a tendency among the falcons (*F. candicans*, *F. islandus*, &c.) to become white in high latitudes—a mode of coloration which does not appear to me to be of much use in such species. These birds, as far as I know, swoop down on their prey from above, under which circumstances the lighter colouring would be of no advantage in enabling them to approach their prey undetected; on the other hand, it can hardly be maintained that these birds are subject to any persecution which would cause their lighter plumage to be of protective value. When on the wing the back only would be seen by another bird hovering over the falcon, and it is noteworthy that this part of the falcons in question is darker than the under side. The same considerations apply to the snowy owl (*Nyctea scandiaca*). In many other birds, again, such as the plovers (*Charadrius plumialis*, *Squatarola cinerea*, &c.) and various species of *Scolopacidae* (*Tringa variabilis*, *T. subarquata*, &c.), the under side only changes to white in winter—a change which it is impossible to associate either with protection from foes or with predatory advantage. On the other hand, it seems not unreasonable to suppose (on the radiation theory) that the under side of the bird, being nearest to the snow-covered surface of the ground, would require the most protection. It is of interest also to bear in mind from the present point of view that many mammals are known to become white on the under side during winter. Thus, Surgeon-Major Leith Adams, F.R.S., states in his observations on the natural history of Eastern Canada¹ that “there is, moreover, a seemingly strong disposition for the lower parts of animals to become white in winter—i.e. the parts in closest contact with the snow; thus the under surfaces of the deer tribe are always whitest. And, as if from its habit of constantly digging among the snow with its snout in quest of food, we find the cariboo with a white patch on its lips and around the hoof, &c.” Such facts as these cannot, as it appears to me, be explained on the protection theory; but if any connection exists between the mode of colouring of an animal and its external conditions of life, the theory of preventive radiation or even the direct action of low temperature on the formation of the pigment seems to be more applicable.

The objections raised by Signor Camerano, although supported by some interesting observations, are, I venture to think, somewhat wide of the mark. The writer, indeed, endeavours to bring within the scope of the radiation theory classes of facts which I for one should certainly never dream of attributing to this cause, even if it had been demonstrated on a sound experimental basis. There can be no question as to the truth of his concluding statement that the causes tending to modify the colours are of an extremely complex character. It is this very complexity, indeed, which renders it so highly important to thoroughly investigate any explanation which bears the stamp of truth, though perhaps applicable to but a very limited group of facts. In view of these difficulties, and bearing in mind the inexhaustible resources of nature in adapting organisms to their environment by apparently opposite means, it is not at all surprising that cases should exist which stand apparently opposed to the particular class of cases here dealt with. There are many conceivable ways of enabling an animal to struggle against a severe climate besides that of lightening the colour of its fur, and natural selection would take advantage of any and every means presented for securing this end. To say, therefore, that some animals become darker in winter (*Cervus mandchurinus*), or that others do not change colour at all (*Rupicapra europea*, *Capra ibex*), is no real objection to the radiation theory, but simply an illustration of the principle that there are many ways of securing the same result. Thus, in the case of the two last-named species, Sig. Camerano himself states that there is a great difference in the thickness of the winter covering. Then, again, the statement that a more or less distinct seasonal change of colour is observable in many animals appears to me to have no precise bearing on the question—all that can be said from the point of view either of adaptation or climatic protection is that in such slight mutations we have given to us a hint as to the method by which the more striking seasonal changes have been brought about. We must regard such changes either as the incipient stages of a seasonal variation which could, if necessary, be worked up into a more perfect adaptation (*protective or climatic*), or as the vanishing remnants of a seasonal variation formerly important, but now useless. The facts that some animals which are not polar or alpine are *permanently* white, that the

¹ “Field and Forest Rambles,” 1873, p. 124.